

# **EXHIBIT 27**

## Declaration of Gregory K. Leonard

**HIGHLY CONFIDENTIAL UNDER PROTECTIVE ORDER**

**UNITED STATES DISTRICT COURT  
DISTRICT OF NEVADA**

CUNG LE, NATHAN QUARRY, JON FITCH,  
BRANDON VERRA, LUIS JAVIER VASQUEZ,  
and KYLE KINGSBURY, *on behalf of themselves  
and all others similarly situated,*

Plaintiffs,

v.

ZUFFA, LLC D/B/A ULTIMATE FIGHTING  
CHAMPIONSHIP AND UFC,

Defendant.

Case No. 2:15-cv-01045-RFB-BNW

**DECLARATION OF DR. GREGORY K. LEONARD**

**December 1, 2023**

TABLE OF CONTENTS

	<u>Page</u>
I. QUALIFICATIONS.....	2
II. ASSIGNMENT.....	3
III. SUMMARY OF OPINIONS.....	3
IV. GIVEN THE ECONOMICS LITERATURE ON EXCLUSIVE DEALING, DR. SINGER SHOULD HAVE USED AN APPROACH THAT ALLOWS FOR THE POSSIBILITY OF INDIVIDUAL EFFECTS .....	5
V. DR. SINGER’S “IMPACT REGRESSION” IS AN “AGGREGATE” ANALYSIS NOT CAPABLE OF DETERMINING IMPACT FOR ANY INDIVIDUAL FIGHTER.....	6
VI. WHEN DR. SINGER’S IMPACT REGRESSION IS GENERALIZED TO ALLOW EACH FIGHTER TO HAVE A SEPARATE FORECLOSURE SHARE COEFFICIENT, MANY FIGHTERS ARE FOUND NOT TO HAVE BEEN ADVERSELY AFFECTED BY FORECLOSURE SHARE .....	10
VII. DR. SINGER’S “COMPENSATION STRUCTURE” REGRESSION OBSCURES THE FACT THAT MANY FIGHTERS’ COMPENSATION DOES NOT MOVE OVER TIME WITH OTHER FIGHTERS’ COMPENSATION .....	15
VIII. DR. SINGER’S “COMMON FACTORS” REGRESSION MISTAKENLY ATTRIBUTES INDIVIDUALIZED SOURCES OF VARIATION TO COMMON FACTORS .....	25
IX. DR. SINGER’S OTHER PURPORTED METHOD FOR DETERMINING INDIVIDUAL INJURY IS COMPLETELY FLAWED AND UNRELIABLE .....	27
X. CONCLUSION.....	35

## **I. QUALIFICATIONS**

1. My name is Gregory K. Leonard. I am an economist and Vice President at Charles River Associates, 601 12<sup>th</sup> Street, Suite 1500, Oakland, CA 94607.

2. I received a Bachelor of Science in Applied Mathematics-Economics from Brown University in 1985 and a Ph.D. in Economics from the Massachusetts Institute of Technology in 1989. After receiving my Ph.D., I became an assistant professor at Columbia University. I subsequently moved into economic consulting and worked at several economic consulting firms prior to joining CRA.

3. My specialties within economics are applied microeconomics, the study of the behavior of consumers and firms, and econometrics, the application of statistical methods to economics data. I have published more than sixty articles in scholarly and professional publications, which are listed on my curriculum vitae, which is attached as Appendix A. Many of these articles address issues in industrial organization, antitrust, and econometrics. Several also more specifically address issues related to proving antitrust injury and damages.

4. I served as the Vice Chair for Economics of the Board of Editors of the *Antitrust Law Journal* from 2018 to 2023 (and am currently a Senior Editor) and have served as a referee for numerous economics and other professional journals. I have given invited lectures on antitrust issues at the Federal Trade Commission (FTC), the United States Department of Justice (DOJ), the Directorate General for Competition of the European Commission, the Fair Trade Commission of Japan, and China's Supreme People's Court and Ministry of Commerce. I have been retained by the DOJ to consult on antitrust matters.

5. In 2007, I served as a consultant to, and testified before, the Antitrust Modernization Commission, which was tasked by Congress and the President of the United States to make recommendations for revising U.S. antitrust laws.

6. I have served as an expert witness in a number of litigation matters before U.S. District Courts, state courts, arbitration panels, and the U.S. International Trade Commission. A list of cases in which I have testified (in deposition or at trial) in the last five years is provided in Appendix A. My hourly rate for this matter is \$1,200. My or CRA's compensation is not contingent on the outcome of the case.

## **II. ASSIGNMENT**

7. In this declaration, I analyze whether the regression methodology proposed by plaintiffs' expert Dr. Hal Singer is reliable as an approach to determine the answer to the key question of whether individual fighters were injured by the challenged conduct.<sup>1</sup>

8. The materials I have considered in preparing this declaration are listed in Appendix B.

## **III. SUMMARY OF OPINIONS**

9. I have reached the following opinions:

- Plaintiffs have alleged that Defendant Zuffa, LLC ("Zuffa") attempted to exclude rivals by entering into exclusive deals with fighters.<sup>2</sup> Given that the theoretical economics literature

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<sup>1</sup> In this declaration, except as noted, I do not offer any opinions about how Dr. Singer defined variables such as foreclosure share, his choices of dependent variables and explanatory variables to use in his regressions, and his choices regarding fighters and time periods to include in his regressions, among other issues. I understand that Dr. Topel has offered opinions on many of these issues.

<sup>2</sup> My discussion focuses on the alleged exclusionary effects of Zuffa's contracts because, as admitted by Dr. Singer, his impact regression "measures only the exclusionary effects of Zuffa's Fighter contracts, and not the exclusionary effects of other aspects of the Challenged Conduct." Expert Report of Hal J. Singer, Ph.D. (August 31, 2017) ("Singer Report"), n. 456. As I show, Dr. Singer's model is incapable of establishing impact or damages for any individual fighter. I understand that Zuffa does business as the Ultimate Fighting Championship ("UFC").

on anticompetitive exclusive dealing raises the possibility that some dealers benefit or are left uninjured by the conduct, Dr. Singer should have, but did not, use an empirical method for analyzing injury that allowed for the possibility that some fighters were not injured.

- Instead, Dr. Singer’s “impact regression” is an “aggregate” analysis. It does not measure the injury due to the challenged conduct for any individual fighter. Rather it measures injury for the average fighter. The injuries (if any) sustained by individual fighters, while “centered” around the average, may be widely dispersed and may extend to zero (no injury) or negative numbers (benefit) for some fighters. Indeed, as noted above, economic theory suggests it is possible that some individual fighters benefited from the challenged conduct. Dr. Singer’s impact regression says nothing about the distribution of injury due to the challenged conduct across individuals.
- However, given that many fighters are observed over multiple events, Dr. Singer’s impact regression can be altered to allow the foreclosure share variable—his measure of the challenged conduct—to have a separate coefficient for each such fighter, while leaving all other aspects of Dr. Singer’s specification, including his measurement of fighters’ share of event revenue, unchanged. This revised version of Dr. Singer’s impact regression shows that, for a substantial number of fighters, there is no support for the hypothesis that they were injured by increased foreclosure share. A statistical test rejects Dr. Singer’s “average” injury formulation in favor of the “individual injury” formulation. Thus, Dr. Singer’s impact regression is not a good fit for answering the question of injury for any individual fighter.
- Dr. Singer supplements his impact regression with a “compensation structure” regression in which he purports to show that a given fighter’s annual compensation per event moves closely with the average annual compensation per event of other fighters. However, again Dr. Singer’s regression is an “aggregate” analysis that at best shows how the average fighter’s compensation moves with the average compensation of other fighters.<sup>3</sup> Dr. Singer’s focus on averages obscures substantial underlying variation among individual fighters in the extent to which their compensation is correlated with other fighters. Looking at individuals instead of averages, as I have done here, reveals that many pairs of individual fighters have small or negative correlation between their respective annual compensation per event.
- Dr. Singer also attempts to support his claim of common impact by arguing that “common factors” explain a large proportion of the variation in individual fighter compensation.

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<sup>3</sup> Moreover, it focuses on the wrong question. The right question is: does the compensation of individual fighters move together because the challenged conduct injured all fighters in the same way? Because Dr. Singer’s compensation structure regression fails to distinguish among the many reasons why the compensation of individual fighters may move together and, in particular, because it fails to isolate the effect of the challenged conduct (as defined in Dr. Singer’s model), it is incapable of answering the right question.

However, Dr. Singer’s “common factors” regression is flawed in multiple ways. First, his “common factors” in many cases are actually “individual factors” in that they take on different values for different fighters. Second, he again fails to recognize that the effects of these individual factors are themselves individual, not common. That is, different fighters’ compensation may respond differently to the same factor (as demonstrated above with the foreclosure share).

- Finally, Dr. Singer attempts to bolster his claim that all fighters were injured by arguing that his impact regression predicts a but-for fighter share of event revenues that was higher than the actual fighter share of revenue for almost all fighters. However, Dr. Singer’s approach is completely flawed as a matter of economics and statistics because it rests on a crucial assumption—that his model perfectly predicts the but-for fighter share for every fighter and event—that is entirely unsupported and inconsistent with the data. In contrast, the corrected version of Dr. Singer’s impact regression, as described above, refutes Dr. Singer’s assertion that all or substantially all fighters were injured.

#### **IV. GIVEN THE ECONOMICS LITERATURE ON EXCLUSIVE DEALING, DR. SINGER SHOULD HAVE USED AN APPROACH THAT ALLOWS FOR THE POSSIBILITY OF INDIVIDUAL EFFECTS**

10. The basic idea behind economic models of anticompetitive exclusive dealing is that the “incumbent” firm uses exclusive contracts with dealers to foreclose an entrant from gaining the level of scale necessary to be profitable.<sup>4</sup> One question that these models had to address is why dealers rationally would agree to enter into contracts that would ultimately harm them (by foreclosing entry). The models answered this question by identifying the following strategy for the incumbent: offer a subset of dealers an exclusive contract with a price as good or better than the one the dealers could get if entry were to occur. If the subset of dealers receiving this offer is large enough to deny the potential entrant sufficient scale, the incumbent could then offer the other dealers an exclusive contract with a price worse than they would get if entry were to occur.<sup>5</sup> Thus,

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<sup>4</sup> See, e.g., D. Spector, “Exclusive Contracts and Demand Foreclosure,” *RAND Journal of Economics* (2011), pp. 619-638.

<sup>5</sup> An argument that the incumbent could have imposed exclusive licenses because dealers had no alternative firms to choose from at the time of contracting would not undermine these models. The possibility of entry by new or additional firms would continue to support the theoretical models.

under this strategy, there are both “winners” and “losers” among the dealers, even though in “aggregate” or “on average” dealers were adversely impacted.<sup>6</sup>

11. The implication of the theoretical models for this case is that, under plaintiffs’ allegations of exclusive dealing, some fighters (e.g., dealers) may have not been harmed or may have benefited even if fighters were harmed in the aggregate. Given this literature, and the importance for the case of determining injury for individual fighters, Dr. Singer should have used a methodology that explicitly allows for the possibility of different effects of the challenged conduct on different fighters. However, instead, as I discuss in the next section, Dr. Singer employed an aggregate methodology (his “impact regression”). His attempts to re-cast this aggregate methodology as an individual methodology (e.g., his “compensation structure regression”) are all flawed, as I discuss in subsequent sections.

## **V. DR. SINGER’S “IMPACT REGRESSION” IS AN “AGGREGATE” ANALYSIS NOT CAPABLE OF DETERMINING IMPACT FOR ANY INDIVIDUAL FIGHTER**

12. There is a large economics and statistics literature concerned with the estimation of effects of “treatments,” such as government policies and programs, medical procedures and drugs, and so on.<sup>7</sup> Analyzing the effects of a defendant’s challenged conduct on plaintiffs in an antitrust case is an application of the treatment effects framework.<sup>8</sup> The basic approach taken in the treatment

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<sup>6</sup> In the theoretical models, if dealers are unable to coordinate, the incumbent may be able to achieve foreclosure without offering a good deal to any dealer. However, I have seen no evidence that fighters are incapable of coordinating.

<sup>7</sup> A review is provided by G. Imbens and J. Wooldridge, “Recent Developments in the Econometrics of Program Evaluation,” *Journal of Economics Literature*, 2009 (“Imbens and Wooldridge”), pp. 5-86.

<sup>8</sup> See, e.g., J. Johnson and G. Leonard, “Rigorous Analysis of Class Certification Comes of Age,” *Antitrust Law Journal*, 2011, pp. 569-586, at pp. 573-575.

effects literature involves comparing an outcome of interest between a “treatment” group that received the treatment and a “control” group that did not receive the treatment.

13. It has long been recognized in the treatment effects literature that the effect of a treatment can be “heterogeneous” (i.e., vary) across individuals.<sup>9</sup> Often, however, economists are most concerned with evaluating the average effect of the treatment, given that, for example, the decision-makers considering whether to implement a government program are often focused on the program’s average effect. The literature has developed a variety of methods for estimating the average treatment effect.<sup>10</sup> However, these methods, by definition, are not suitable for evaluating the effect on any given individual when effects are heterogeneous. For example, as will be discussed in greater detail below, a regression equation such as the one employed by Dr. Singer in his impact regression that has a single coefficient on the treatment variable that applies to all individuals does not allow one to estimate individual heterogeneous effects.<sup>11</sup>

14. I understand that an important issue in the present case is determining the effect (if any) of the challenged conduct (i.e., injury) on individual fighters. As noted above, treatment effects methods designed to estimate the average treatment effect are not useful for this exercise. For

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<sup>9</sup> See, e.g., Imbens and Wooldridge, p. 7 (“One of the attractions of the [treatment effects framework] is that from the outset it allows for general heterogeneity in the effects of the treatment. Such heterogeneity is important in practice...”).

<sup>10</sup> Imbens and Wooldridge, p. 15.

<sup>11</sup> See, e.g., Imbens and Wooldridge, p. 10, describing the difficulty of interpretation of a regression equation such as Dr. Singer’s impact regression (“Often, researchers write down a regression function  $Y_i = \alpha + \tau \cdot W_i + \varepsilon_i$ . This regression function is then interpreted as a structural equation, with  $\tau$  as the causal effect. Left unclear is whether the causal effect is constant or not [i.e., heterogeneous], and what the properties of the unobserved component,  $\varepsilon_i$ , are.”) and pp. 14-15 (“A key feature of the current literature, and one that makes it more important to be precise about the questions of interest, is the accommodation of general heterogeneity in treatment effects. In contrast, in many early studies it was assumed that the effect of a treatment was constant [i.e., not heterogeneous], implying that the effect of various policies could be captured by a single parameter. The essentially unlimited heterogeneity in the effects of the treatment allowed for in the current literature implies that it is generally not possible to capture the effects of all policies of interest in terms of a few summary statistics.”).

analyzing individual effects, the literature has developed other methods.<sup>12</sup> I now explain in detail how Dr. Singer’s “impact regression” at best estimates the average effect of the challenged conduct and is not appropriate for determining individual effects (i.e., injury).

15. Dr. Singer estimates a regression model in which he relates a fighter’s compensation expressed as a percentage of event-revenue (“Fighter Share,” “Wage Share,” or “Revenue Share”) on his calculation of Zuffa’s weighted “foreclosure share” and “other factors that may influence Fighter-compensation shares.”<sup>13</sup> For each of the explanatory variables in his regression model, including the foreclosure share, Dr. Singer estimates a single coefficient.<sup>14</sup> The single coefficient on foreclosure share (or any other explanatory variable) has two possible mutually exclusive interpretations: (1) the effect of foreclosure share is the same for every individual fighter and the coefficient represents this effect; or (2) the effect of foreclosure share differs across individual fighters and the coefficient represents the average of the individual fighters’ effects.<sup>15</sup>

16. Under the second interpretation, the single coefficient that Dr. Singer’s impact regression estimates (the average effect) cannot be used to determine whether any individual fighter was

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<sup>12</sup> See, e.g., Imbens and Wooldridge, p. 50 (describing testing methods used when “there is substantive interest in whether the program is beneficial for some groups, even if on average it does not affect outcomes.” (footnote omitted)); M. Campello, et al., “Testing for Slope Heterogeneity Bias in Panel Data Models,” *Journal of Business and Economic Statistics*, 2019, pp. 749-760 (“Campello, et al.”), at p. 749 (“This article characterizes and addresses the issues of estimation and inference of econometric models in the presence of heterogeneity in individual policy responses (‘slope coefficients’). We contribute to the literature by proposing the use of methods that allow researchers to easily identify, quantify, and address estimation issues arising from individual heterogeneity. More generally, our analysis warns researchers about imposing arbitrary homogeneity restrictions when studying complex economic behavior.”).

<sup>13</sup> Singer Report, ¶¶181-182, 184. See also, generally, Singer Report, §III.D.1.

<sup>14</sup> In alternative models, Dr. Singer allows either his foreclosure share or other explanatory variables to vary between Zuffa fighters and pre-acquisition Strikeforce fighters. See, e.g., Singer Report n. 454 (allowing the foreclosure share to vary) and Rebuttal Expert Report of Hal J. Singer, Ph.D. (January 12, 2018) (“Singer Rebuttal Report”) ¶85 and Table A1 (allowing other explanatory variables to vary between Zuffa bouts and Strikeforce pre-acquisition bouts). I note that these regressions are still aggregate models that do not allow the effect to vary across individual Zuffa fighters.

<sup>15</sup> See Imbens and Wooldridge, p. 10 concerning the interpretation. See also, M. Campello, et al., p. 751.

injured (that individual's own effect). To see why, consider the following example. There are three fighters and, as under the second interpretation, each of the fighters has his or her own foreclosure share effect (on the Fighter Share). Assume those effects are -0.04, 0, and 0.01, respectively. Note that the second and third fighters were not injured by any increase in foreclosure share. However, Dr. Singer's impact regression would (approximately) estimate a coefficient equal to the average effect, i.e., the average of -0.04, 0, and 0.01, or -0.01. If one attempted to use this "aggregate" coefficient as a guide to injury for individual fighters, one would mistakenly conclude that all three fighters were injured when only one actually was injured.<sup>16</sup>

17. Thus, if one is contemplating using Dr. Singer's impact regression to assess injury for individual fighters, it is crucially important to determine which of the interpretations applies.<sup>17</sup> If the second interpretation applies, Dr. Singer's impact regression is not an appropriate fit to the question at hand. Which interpretation applies is an empirical question that must be addressed using econometric methods.

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<sup>16</sup> Dr. Singer in fact acknowledges that his impact regression produces an average effect. In interpreting his coefficient on Zuffa's foreclosure share, he explains "if the *Foreclosure Share* were to decrease from 90 percent to 80 percent, the Fighter Share for the **average** Fighter would increase by  $(0.9-0.8) \times (0.0319) = 0.00319$ , or about 0.319 percentage points... Given that the **average** Fighter Share is only about 1.2 percent (as seen in Table 5 above), the **average** Fighter's compensation would increase by about 27 percent (equal to  $0.00319/0.012$ ) if the *Foreclosure Share* were to decrease from 90 percent to 80 percent." Singer Report, ¶187, emphasis added.

<sup>17</sup> In fact, which interpretation is correct can affect the ability of a regression such as Dr. Singer's to estimate the average effect reliably, making it all the more important to investigate which interpretation is correct. See, e.g., Campello, et al., p. 751 ("If all individuals are identical [have the same effect], then [regression with a single coefficient for all individuals] provides an easy way to estimate the parameters of interest. It is rarely the case, however, that one can a priori justify the assumption of homogeneity in individuals' responses to economic stimuli. Indeed, as discussed above, theoretical modeling and casual observation often suggest otherwise. We now characterize the problems of using the [regression] to estimate the population [effect] (the average individual slope coefficients) in the presence of policy heterogeneity.").

**VI. WHEN DR. SINGER'S IMPACT REGRESSION IS GENERALIZED TO ALLOW EACH FIGHTER TO HAVE A SEPARATE FORECLOSURE SHARE COEFFICIENT, MANY FIGHTERS ARE FOUND NOT TO HAVE BEEN ADVERSELY AFFECTED BY FORECLOSURE SHARE**

18. As explained above, under the second interpretation of the foreclosure share coefficient, Dr. Singer's regression model is only capable at best of measuring the *average* effect, across all fighters, of foreclosure share.<sup>18</sup> Dr. Singer's impact regression, as designed, does not allow for the possibility that the effect of foreclosure share differed across fighters and some fighters may not have been undercompensated (even using Dr. Singer's Revenue Share compensation proxy). Dr. Singer did not statistically test which interpretation of the foreclosure share coefficient is correct, despite this issue being crucial for his claim of being able to demonstrate individual injury.<sup>19</sup>

19. I performed generally accepted statistical tests of Dr. Singer's implicit assumption that the first interpretation is correct, i.e., that the effect of foreclosure share is the same for all fighters.<sup>20</sup>

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<sup>18</sup> *Id.*

<sup>19</sup> *Id.*

<sup>20</sup> The statistical test I use is the well-accepted *F*-test for joint significance. Specifically, I test the joint hypothesis that the foreclosure share coefficients for all fighters are equal. *See, e.g., J. Wooldridge, Introductory Econometrics: A Modern Approach*, 7<sup>th</sup> edition, Cengage Learning ("Wooldridge"), pp. 139-144. The statistical test demonstrates that not allowing foreclosure share to have an effect that varies across fighters is a flawed approach. Dr. Singer himself used a similar test to see whether the foreclosure share effect was the same for pre-acquisition Strikeforce fighters as for Zuffa fighters. Dr. Singer explains "[m]y initial report used a standard *F*-test to confirm 'that Zuffa's foreclosure affected the Fighter Shares of Zuffa Fighters, but not Strikeforce pre-acquisition Fighters' [citing Singer Report, n. 454]. This *F*-test is simply a variant of the Chow test of the hypothesis that the slope coefficient on Zuffa's foreclosure share is identical for Zuffa bouts and Strikeforce pre-acquisition bouts." Singer Rebuttal Report, n. 297. Indeed, in Dr. Singer's opening report he presents an alternative model which allows his foreclosure share to vary between pre-acquisition Strikeforce fighters and Zuffa fighters. Specifically, he interacts his foreclosure share with a dummy variable indicating if the Fighter was Zuffa-owned or a pre-acquisition Strikeforce fighter. Singer Report, n. 454. He concludes that "[t]he coefficient  $\alpha$  [on the interaction of foreclosure share and his pre-acquisition Strikeforce indicator] was found to be statistically insignificant, indicating that Zuffa's foreclosure affected the Fighter Shares of Zuffa Fighters, but not Strikeforce pre-acquisition Fighters." *Id.*

Those tests strongly reject Dr. Singer's assumption.<sup>21</sup> The test results demonstrate that foreclosure share does not have the same effect across all fighters. Moreover, when I use Dr. Singer's impact regression models and allow for his coefficient on foreclosure share to vary across fighters, while making no other changes to the models, I find that for a substantial portion of fighters there is no negative and statistically significant relationship between the foreclosure share (as measured by Dr. Singer) and Fighter Share. Figure 1 provides a summary of the results. Figure 1 shows that, for more than 80% of fighters, the regression results do not support the claim that Fighter Share was suppressed as a result of the challenged conduct (as measured by Dr. Singer's foreclosure share).<sup>22</sup>

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<sup>21</sup> The *F*-statistic is 35,909 and *p*-value is <0.00001% for Dr. Singer's Tracked definition of foreclosure share,  $1.0 \times 10^8$  and <0.00001% for his Ranked definition, and  $1.3 \times 10^5$  and <0.00001% for his Headliner (Revenue Weighted) definition. Consistent with the regression results Dr. Singer presents in his reports and backup materials, my testing using Dr. Singer's "Headliner" foreclosure share definition is based on Dr. Singer's calculation of the Headliner foreclosure share weighted by revenue.

While some individual fighters have few bouts (and thus, few observations), any argument that the coefficient on foreclosure share cannot be estimated precisely across individual fighters due to small sample size would be incorrect as a matter of fundamental scientific and statistical practice. The sample sizes (and resulting statistical precision) are sufficient to reject Dr. Singer's averaging across all fighters, as demonstrated by the *F*-test. Regardless, I also ran a version of the regression where I allowed the foreclosure share effects to vary across each fighter with at least 10 bouts and assumed that the foreclosure share effect was the same for the remaining fighters, see below.

<sup>22</sup> I limit the fighters summarized in the table to only the Bout Class Members in Dr. Singer's regression data, consistent with those reported by Dr. Singer (see, e.g., Singer Rebuttal Report, Table 2, indicating 1,056 total fighters). As shown in the table, 117 fighters do not have enough datapoints (bouts) to generate an estimate. Among fighters with enough datapoints for the regression to produce an estimate (i.e., excluding these fighters from the numerator as well as the denominator), over 80% do not have a negative and statistically significant slope coefficient on the foreclosure share.

**Figure 1**  
**Summary of Estimated Foreclosure Share Coefficients Using Dr. Singer's Impact Regression And Allowing Foreclosure Share to Vary For Each Fighter**

	<b>Tracked</b>		<b>Ranked</b>		<b>Headliner</b>	
	Count of Fighters	Percentage	Count of Fighters	Percentage	Count of Fighters	Percentage
Negative and Statistically Significant	168	16%	178	17%	152	14%
Positive and Statistically Significant	128	12%	68	6%	77	7%
Negative and Not Statistically Significant	441	42%	504	48%	487	46%
Positive and Not Statistically Significant	202	19%	189	18%	223	21%
Not Estimated	117	11%	117	11%	117	11%
<b>Total Fighters with No Negative and Statistically Significant Foreclosure Share</b>	<b>888</b>	<b>84%</b>	<b>878</b>	<b>83%</b>	<b>904</b>	<b>86%</b>
<b>Total Individual Fighters</b>	<b>1,056</b>		<b>1,056</b>		<b>1,056</b>	

*Notes:*

- [1] Foreclosure share has been interacted with all fighters. Fighter counts are limited to fighters in the Bout Class.
- [2] Statistical significance is reported at the 5% percent significance level.

*Source:*

- [a] Dr. Singer's Regression Data.

20. To focus on fighters that have a relatively large number of datapoints, I alternatively allow only those fighters with at least 10 bouts to each have a separate coefficient on foreclosure share in Dr. Singer's impact regression (while all other fighters are assumed to have the same coefficient). The statistical test again strongly rejects the hypothesis that all fighters have the same foreclosure share coefficient.<sup>23</sup> Figure 2 provides a summary of the results. For more than 90% of fighters, the results do not support the claim that Fighter Share was suppressed as a result of the challenged conduct.<sup>24</sup>

<sup>23</sup> The *F*-statistic for Dr. Singer's Tracked, Ranked, and Headliner foreclosure definitions, respectively, are 33,921, 27,287, and 43,744, and the *p*-value is <0.0001% for each definition.

<sup>24</sup> The count and percentages reflected in these figures account for the individual fighters with fewer than 10 bouts, for which a single coefficient is estimated. Limiting the calculations to only fighters who fought at least 10 bouts, for over 65% the results do not support the claim that Fighter Share was suppressed as a result of the challenged conduct.

**Figure 2**  
**Summary of Estimated Foreclosure Share Coefficients Using Dr. Singer's Impact Regression And Allowing Foreclosure Share to Vary For Each Fighter With At Least 10 Bouts**

	Tracked		Ranked		Headliner	
	Count of Fighters	Percentage	Count of Fighters	Percentage	Count of Fighters	Percentage
Negative and Statistically Significant	75	7%	62	6%	67	6%
Positive and Statistically Significant	20	2%	10	1%	19	2%
Negative and Not Statistically Significant	928	88%	962	91%	938	89%
<i>Fighters With At Least 10 Bouts</i>	<i>91</i>	<i>9%</i>	<i>125</i>	<i>12%</i>	<i>101</i>	<i>10%</i>
<i>All Other Fighters Combined</i>	<i>837</i>	<i>79%</i>	<i>837</i>	<i>79%</i>	<i>837</i>	<i>79%</i>
Positive and Not Statistically Significant	33	3%	22	2%	32	3%
Not Estimated	0	0%	0	0%	0	0%
<b>Total Fighters with No Negative and Statistically Significant Foreclosure Share</b>	<b>981</b>	<b>93%</b>	<b>994</b>	<b>94%</b>	<b>989</b>	<b>94%</b>
<b>Total Individual Fighters</b>	<b>1,056</b>		<b>1,056</b>		<b>1,056</b>	

*Notes:*

- [1] Foreclosure share has been interacted with all fighters with at least 10 bouts and all other fighters combined. Fighter counts are limited to fighters in the Bout Class.
- [2] Of the 1,056 fighters, 219 fighters fought in at least 10 bouts. A single coefficient is estimated for the remaining 837 fighters with fewer than 10 bouts.
- [3] Statistical significance is reported at the 5% percent significance level.

*Source:*

- [a] Dr. Singer's Regression Data.

21. To summarize, these results lead to three conclusions. First, the effect of foreclosure share is not the same for all fighters; Dr. Singer's single coefficient on foreclosure share at best represents only the average of the individual fighter-level effects. Second, for the majority of fighters, the results do not support the conclusion that foreclosure share had an adverse effect on their Fighter Shares. Third, Dr. Singer's impact regression, with its single coefficient on foreclosure share, cannot be used to determine injury for any individual fighter.

22. To the extent that Dr. Singer argues that the version of his impact regression that I have presented here—allowing for separate foreclosure share effects for each fighter—could be used to determine injury for individual fighters, he would be incorrect for two reasons. First, there are 117

fighters in Dr. Singer's impact regression who are Bout Class members (over 11% of the total number of Bout Class fighters in the regression) that have only a single observation in Dr. Singer's regression and therefore have insufficient data to estimate their individual foreclosure share effects. As Dr. Singer himself explains, his regression identifies the foreclosure share coefficient from "two sources of variation: (1) variation in Zuffa's own compensation practices over time ... and (2) variation between Zuffa's compensation practices and those of Strikeforce before Zuffa acquired it."<sup>25</sup> The fighters with only a single observation in Dr. Singer's regression have no variation across either of these dimensions. Because the fighters for whom there is sufficient data to estimate their individual effects demonstrate a mix of positive and negative effects, there is no basis in Dr. Singer's impact regression (even the version I have presented) to conclude anything about injury for the fighters with only one bout.<sup>26</sup>

23. Second, the fact that the effect of foreclosure share varies across individual fighters raises the possibility that the effects of other explanatory variables in Dr. Singer's impact regression model also vary across individual fighters. In that case, Dr. Singer's impact regression would have to be abandoned in favor of even more individualized regression models. To illustrate this point, I identified the three explanatory variables (other than foreclosure share) in Dr. Singer's impact regression that are the most "statistically significant" as measured by their t-statistics (*Fight Of*

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<sup>25</sup> Singer Rebuttal Report, ¶3.

<sup>26</sup> Furthermore, in allowing Dr. Singer's foreclosure share to vary across fighters with at least 10 bouts and all other fighters combined, the foreclosure share effect for all other fighters combined, consisting of over 830 individual fighters in the Bout Class, does not support the conclusion that these fighters were injured. See Figure 2.

*The Night, Fighter Performance Of The Night, and Strikeforce*).<sup>27</sup> I then allowed the effects of these three explanatory variables to vary across all fighters, as well as across fighters with at least 10 bouts and all other fighters combined in the same way that I did with foreclosure share.<sup>28</sup> As with foreclosure share, statistical tests reject the hypothesis (implicit in Dr. Singer's impact regression model) that these three explanatory variables have the same effects for all fighters.<sup>29</sup> This result supports the conclusion that Dr. Singer's impact regression is unable to determine injury for any individual fighter without further individualizing the regression model (beyond just the individualization of the foreclosure share effect).

## **VII. DR. SINGER'S "COMPENSATION STRUCTURE" REGRESSION OBSCURES THE FACT THAT MANY FIGHTERS' COMPENSATION DOES NOT MOVE OVER TIME WITH OTHER FIGHTERS' COMPENSATION**

24. Dr. Singer puts forward an analysis of a purported pricing structure in which he "performed regressions to determine whether gains (or losses) in compensation are broadly shared across the Bout Class."<sup>30</sup> Specifically, he claims to test whether a fighter's average annual per-event compensation is correlated with (1) the average per-event compensation paid to other fighters in

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<sup>27</sup> I excluded from consideration the categorical regressors with large numbers of groups because of the large number of coefficients involved when allowing them to have separate coefficients for each fighter. The point I am making could only become stronger if a statistical test indicated that those regressors also should have separate coefficients for each fighter.

<sup>28</sup> I run Dr. Singer's model, interacting fighters with the selected explanatory variable, separately for each selected explanatory variable.

<sup>29</sup> The *F*-statistics for the interactions for all fighters with *Fight Of The Night*, *Fighter Performance Of The Night*, and *Strikeforce*, respectively, are 32,487.8, 4,409.2, and 6,994.6 for Dr. Singer's Tracked definition, 19,943, 4,346.4, and 7,027.6 for Dr. Singer's Ranked definition, and 28,331.6, 4,428.2, and 6,990.5 for Dr. Singer's Headliner definition. The *p*-value is <0.00001% in all cases.

The *F*-statistics for the interactions for fighters with at least 10 bouts and all other fighters combined with *Fight Of The Night*, *Fighter Performance Of The Night*, and *Strikeforce*, respectively, are 754.9, 337.5, and 1,913.5 for Dr. Singer's Tracked definition, 767.9, 331.6, and 1,919.8 for Dr. Singer's Ranked definition, and 747.1, 338.2, and 1,912.8 for Dr. Singer's Headliner definition. The *p*-value is <0.00001% in all cases.

<sup>30</sup> Singer Report, ¶228.

the same year and (2) the average per-event compensation paid to other fighters in the prior year.<sup>31</sup> He finds that “Fighter’s annual compensation per event is statistically significantly associated with the per-event compensation paid to other Fighters” whether he uses other fighters’ compensation either in the same year or in the prior year.<sup>32</sup> He concludes that “individual Fighter compensation per event moves together with the per-event compensation paid to other Fighters, both within and across years, providing further evidence of a pricing structure and, accordingly, of common impact flowing from generalized compensation suppression.”<sup>33</sup> In other words, it is through this “compensation structure” regression that Dr. Singer attempts to show that the average analysis of his impact regression is reflective of the individual fighters’ experience. However, this regression does nothing of the sort.

25. As an initial matter, Dr. Singer’s logic is flawed. His compensation structure regression does not address the relevant question of whether all fighters’ compensation moved in the same direction (i.e., downward) in response to the specific factor that is at issue in this case—the challenged conduct, which he purports to measure with the foreclosure share. Rather, as Dr. Singer himself acknowledges, the compensation structure regression measures whether changes in compensation “broadly,” i.e., due to any factor, not just the challenged conduct, are “shared” across fighters.<sup>34</sup> Fighter compensation may change over time in response to changes in a variety of factors not having to do with the challenged conduct, such as general inflation, greater demand for MMA events, etc. Dr. Singer’s compensation structure regression does not distinguish among

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<sup>31</sup> The only other controls in Dr. Singer’s compensation structure analysis are fighter fixed effects and an annual time trend. Singer Report, ¶228.

<sup>32</sup> *Id.*, ¶229.

<sup>33</sup> *Id.*

<sup>34</sup> *Id.*, ¶¶228-229.

such factors (other than a generic time trend) and specifically does not isolate fighter compensation movements in response to the challenged conduct. There is no basis to believe that the compensation of all fighters moves in the exact same way in response to all factors, and thus no reason to believe that compensation would move in the exact same way in the but-for world in which Zuffa had a different contractual and compensation structure.

26. Moreover, in the previous section, I addressed the relevant question by analyzing whether movements of Fighter Share in response to the challenged conduct (i.e., Dr. Singer's measure of foreclosure share) were the same across fighters; I found that they were not. This result demonstrates the falsity of Dr. Singer's claim that his compensation structure regression shows that his impact regression can be used to demonstrate injury for each individual fighter. Dr. Singer's claimed finding of a compensation structure in fact must be being driven by factors other than the challenged conduct. Accordingly, the compensation structure regression is irrelevant for the question of whether Dr. Singer's impact regression can be used to determine injury to all individual fighters.

27. Furthermore, as with his impact regression, Dr. Singer's compensation structure regression again obscures substantial variation among individual fighters because it essentially is looking at the average correlation of a fighter's compensation with the average compensation of other fighters. Again, Dr. Singer uses a model based on averages to draw conclusions about individual fighters without first asking whether the averages obscure important individual variation. It is more informative to examine the full set of correlations between pairs of individual fighters.<sup>35</sup> If

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<sup>35</sup> My point here is that looking at the correlations between pairs of individuals is more informative than looking at the average across individuals of their correlations with the average of other individuals. I do not, however, endorse the use of correlations to determine whether there is a "structure."

there were a “perfect” compensation structure, all of these correlations would be 1. I started with the individual fighters’ compensation and removed variation due to individual fixed effects and the time trend that Dr. Singer included in his compensation structure regression.<sup>36</sup> I then calculated the correlations of the demeaned and detrended compensation between each pair of fighters. This results in 247,413 correlations.<sup>37</sup> Figure 3 presents the distribution of these correlations. A large number of correlations are -1 or 1, which reflects cases where a pair of fighters has two years in common; when there are two data points on which the correlation is calculated, the correlation is either -1 (the compensation of the two fighters moved in opposite directions between the two years) or 1 (the compensation of the two fighters moved in the same direction). For pairs of fighters with more than two years in common, approximately half have correlations that are positive, but small (e.g., less than 0.10), or negative.<sup>38</sup> I also calculated for each individual fighter the average and median correlation he or she has with other fighters. Figure 4 and Figure 5 presents the distribution of these individual-level averages (Figure 4) or medians (Figure 5). The conclusion to be drawn from these figures is that the average or median correlation for many fighters is less than zero. This variation among individual fighters in the extent to which their compensation is correlated with other fighters’ compensation, and specifically the large number of fighters for whom there is little or no correlation with other fighters’ compensation, is obscured by Dr. Singer’s compensation

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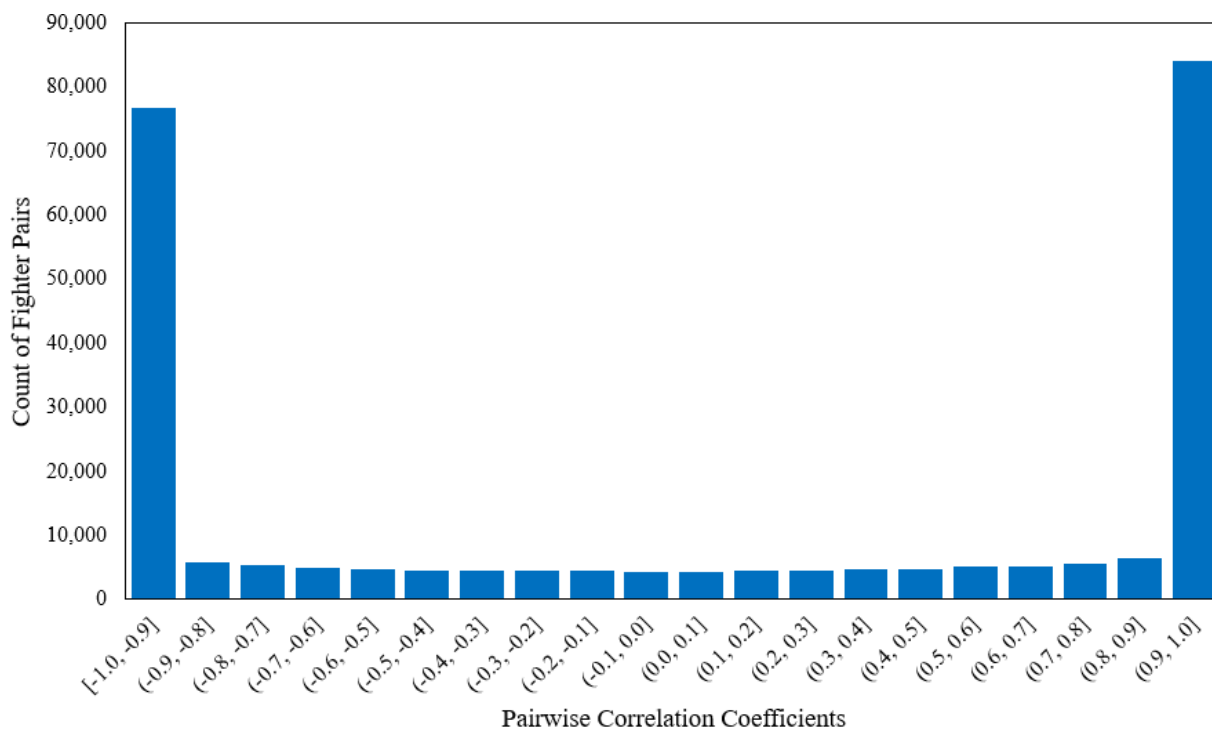
<sup>36</sup> This is analogous to what was done by Dr. Singer’s compensation structure regression, leaving the same variation in compensation that drives Dr. Singer’s estimation of the coefficient on the average of other fighters’ compensation.

<sup>37</sup> A correlation cannot be calculated for any pair of fighters that have only one year of data in common.

<sup>38</sup> My conclusions are the same regardless of where I include or exclude pairs of fighters with only two years in common. Negative correlations can be driven by the zero-sum aspect of contests, among other reasons. The winning fighter receives greater compensation and the losing fighter receives lesser compensation. Dr. Singer’s compensation structure regression does not grapple with this issue at all.

structure regression. Moreover, the substantial number of fighters who have little correlation with other fighters is inconsistent with Dr. Singer's claim of a compensation structure.

**Figure 3**  
**Correlations in Compensation Between Pairs of Fighters**



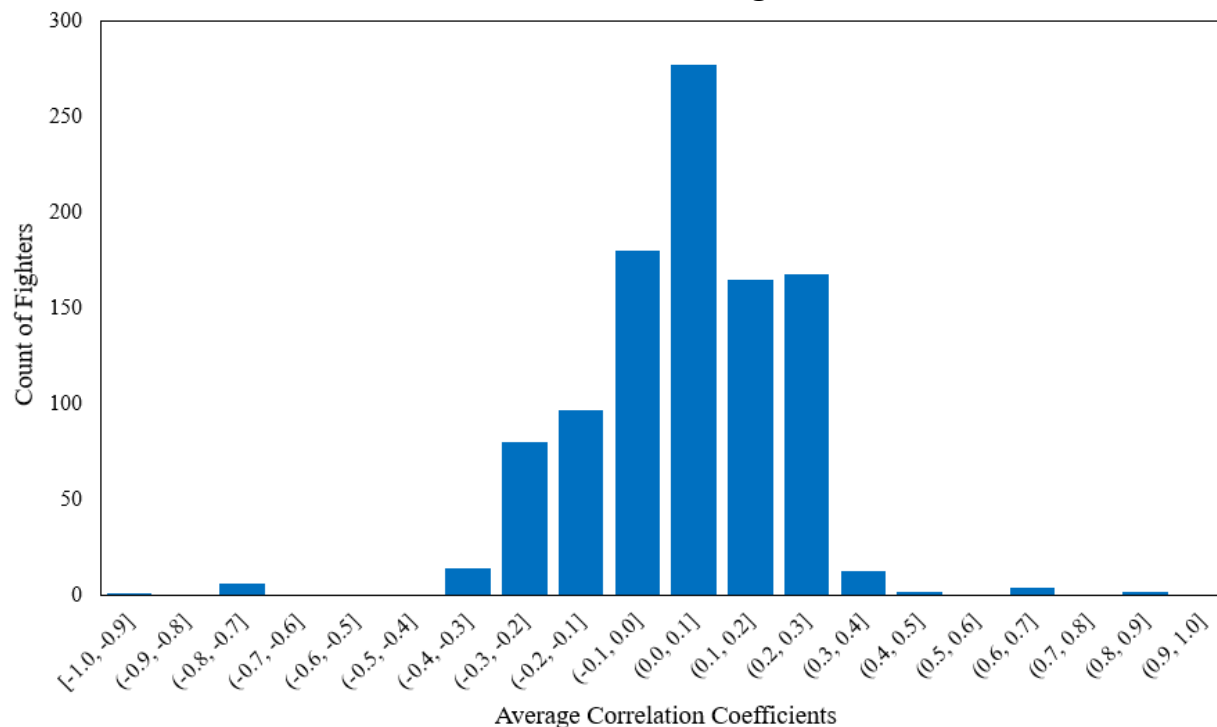
*Notes:*

- [1] Variation due to individual fixed effects and the time trend that Dr. Singer included in his compensation structure regression are removed from the individual fighters' compensation before calculating the correlations.

*Source:*

- [a] Dr. Singer's Regression Data.

**Figure 4**  
**Average of Each Fighter's**  
**Correlations With Other Fighters**



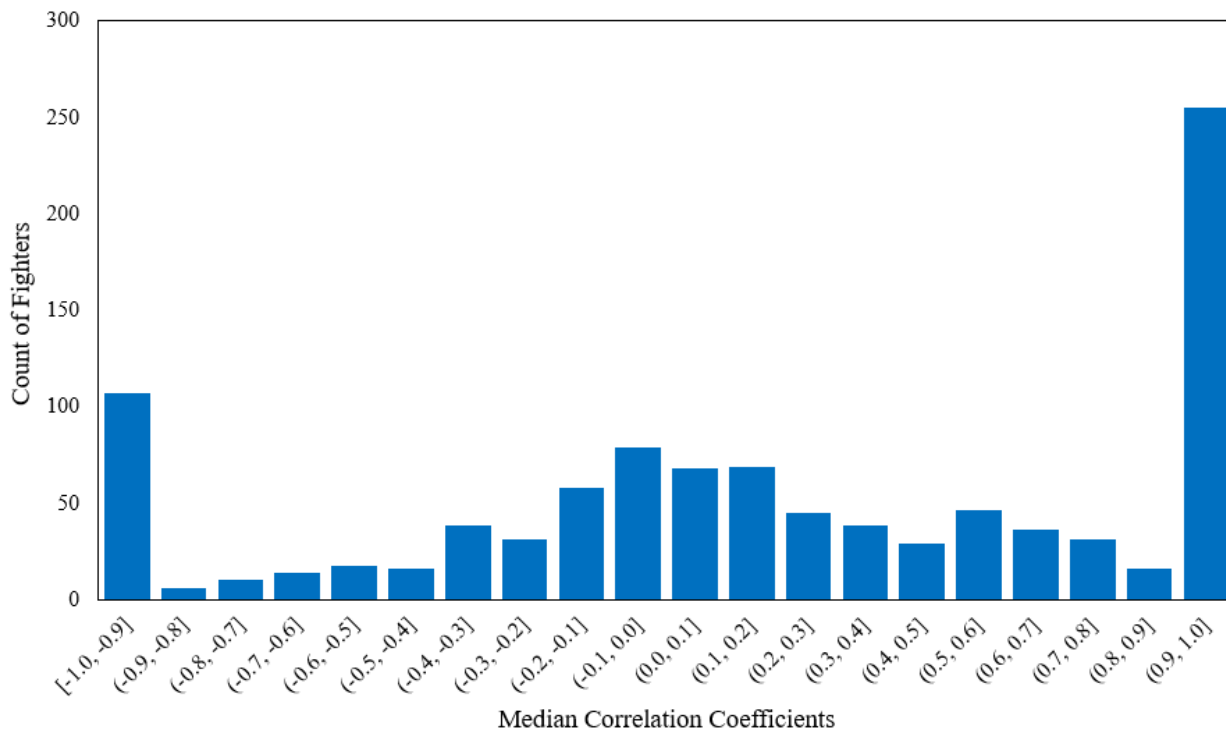
*Notes:*

- [1] Variation due to individual fixed effects and the time trend that Dr. Singer included in his compensation structure regression are removed from the individual fighters' compensation before calculating the correlations.
- [2] Data reflect the average correlation each individual fighter has with other fighters.

*Source:*

- [a] Dr. Singer's Regression Data.

**Figure 5**  
**Median of Each Fighter's**  
**Correlations With Other Fighters**



*Notes:*

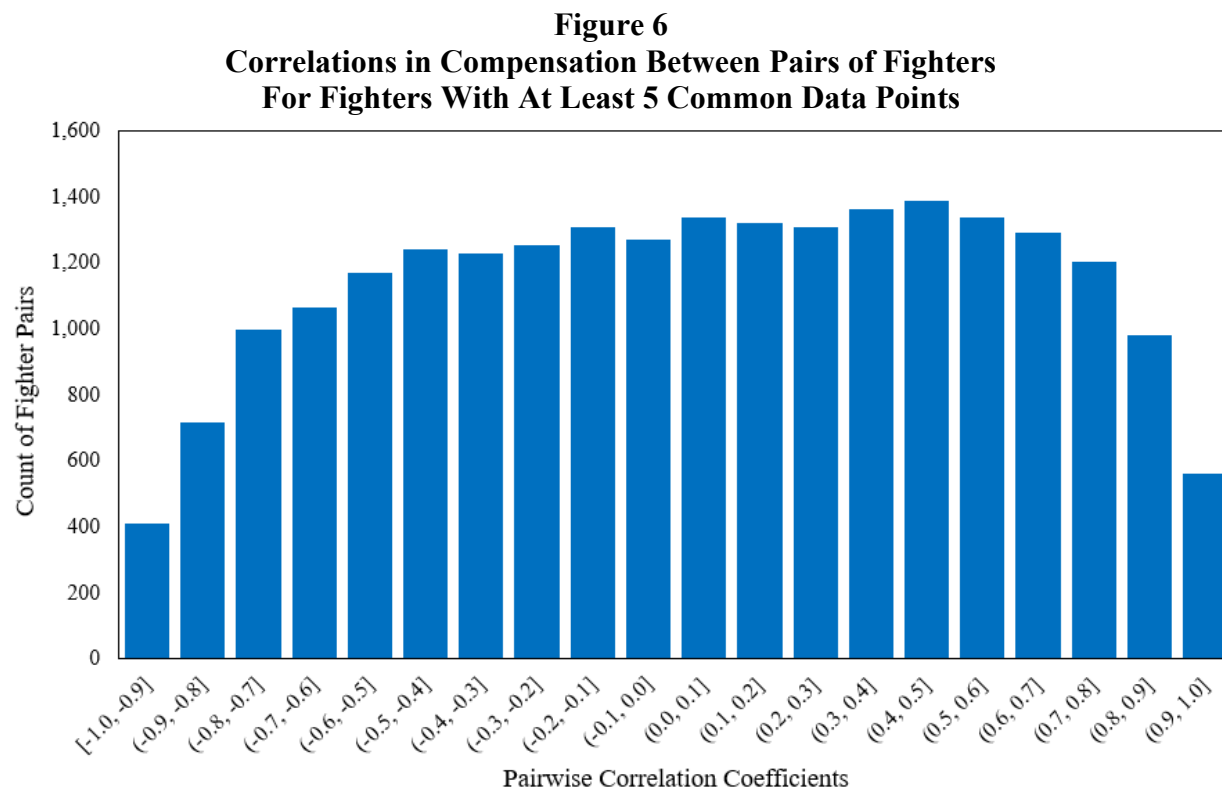
- [1] Variation due to individual fixed effects and the time trend that Dr. Singer included in his compensation structure regression are removed from the individual fighters' compensation before calculating the correlations.
- [2] Data reflect the median correlation each individual fighter has with other fighters.

*Source:*

- [a] Dr. Singer's Regression Data.

28. I also calculated the correlations limited to those pairs of fighters who have at least five years of data in common. This results in 22,737 correlations. The figures below present the distribution of these correlations between fighter pairs as well as the distribution of the average and median correlation each individual fighter has with other fighters. The restriction to pairs of fighters with at least five years in common does not change the picture. Again, approximately half of the fighter pair correlations and over half of the individual-level averages are positive, but small (e.g., less than 0.10), or negative. Thus, my conclusion that Dr. Singer's compensation structure regression obscures variation among individual fighters in the extent to which their compensation

is correlated with other fighters' compensation also holds for this subset of fighters with substantial datapoints in common.



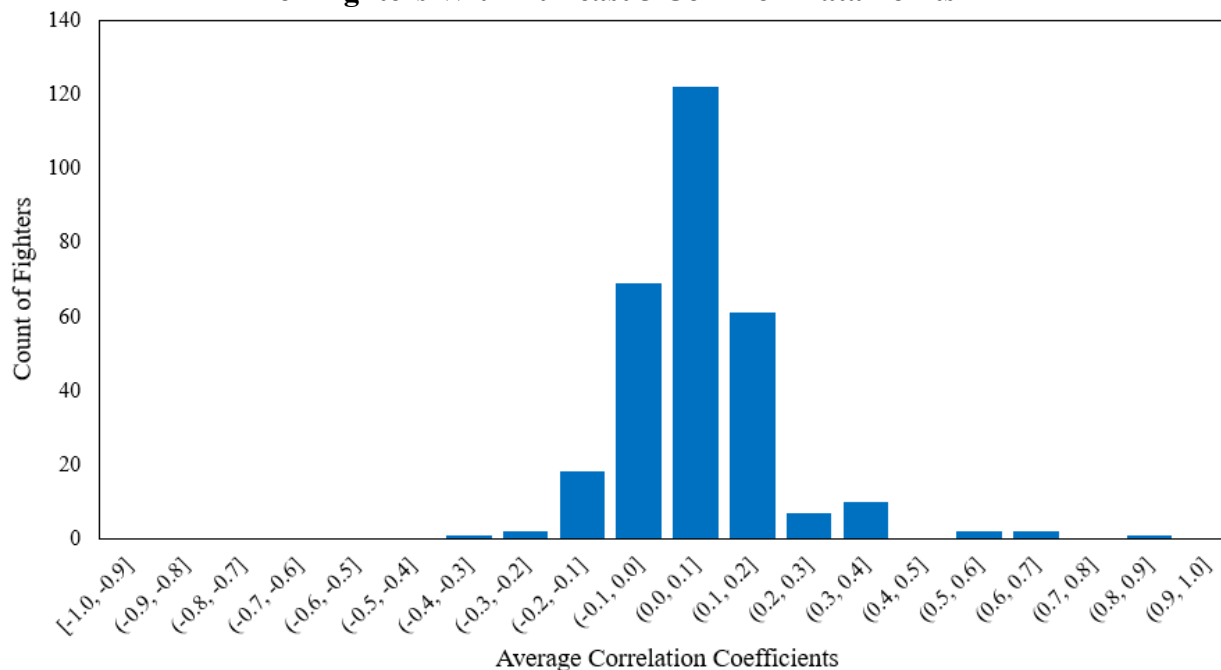
*Notes:*

- [1] Variation due to individual fixed effects and the time trend that Dr. Singer included in his compensation structure regression are removed from the individual fighters' compensation before calculating the correlations.
- [2] Correlations are limited to each pair of fighters who have at least 5 years of data in common.

*Source:*

- [a] Dr. Singer's Regression Data.

**Figure 7**  
**Average of Each Fighter's**  
**Correlations With Other Fighters**  
**For Fighters With At Least 5 Common Data Points**



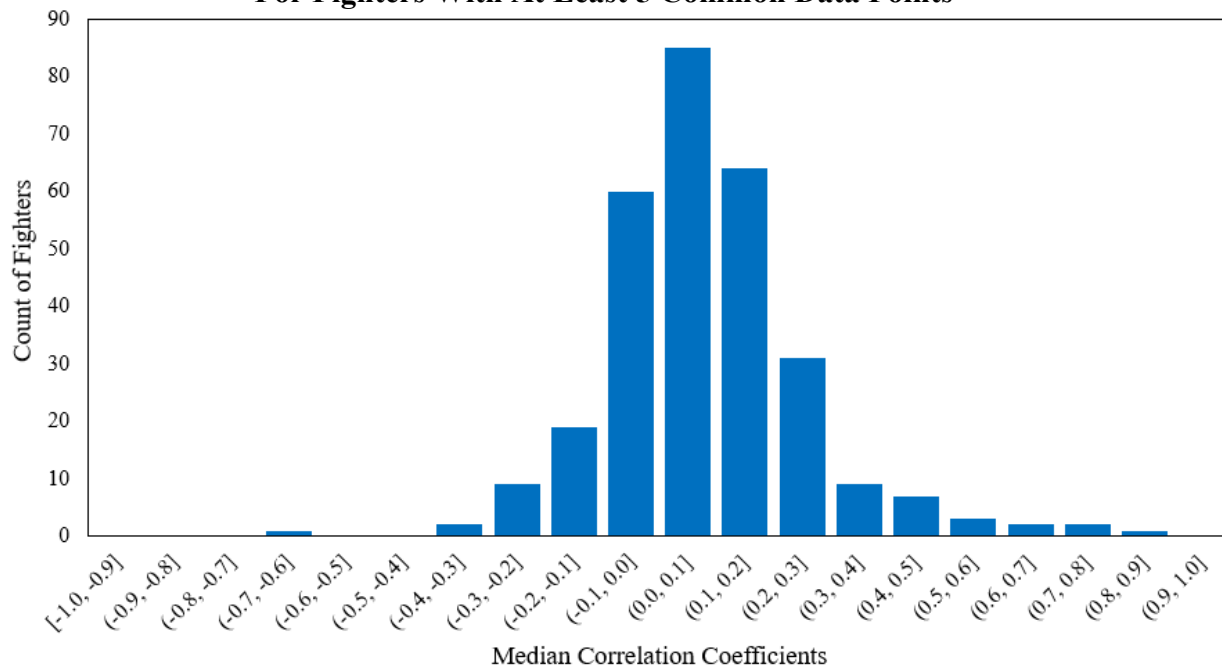
*Notes:*

- [1] Variation due to individual fixed effects and the time trend that Dr. Singer included in his compensation structure regression are removed from the individual fighters' compensation before calculating the correlations.
- [2] Correlations are limited to each pair of fighters who have at least 5 years of data in common.
- [3] Data reflect the average correlation each individual fighter has with other fighters.

*Source:*

- [a] Dr. Singer's Regression Data.

**Figure 8**  
**Median of Each Fighter's**  
**Correlations With Other Fighters**  
**For Fighters With At Least 5 Common Data Points**



*Notes:*

- [1] Variation due to individual fixed effects and the time trend that Dr. Singer included in his compensation structure regression are removed from the individual fighters' compensation before calculating the correlations.
- [2] Correlations are limited to each pair of fighters who have at least 5 years of data in common.
- [3] Data reflect the average correlation each individual fighter has with other fighters.

*Source:*

- [a] Dr. Singer's Regression Data.

29. The variation in correlations across pairs of fighters is not surprising given that, in Dr. Singer's compensation structure regression, "average other fighter compensation" does not explain much of the variation in annual fighter compensation. Comparing Dr. Singer's regression with and without "average other fighter compensation," I find that the addition of "average other fighter compensation" incrementally accounts for less than 1% of the variation in annual fighter compensation, increasing the "within R-squared" from 0.294 to only 0.300; the remaining 70% of variation is unexplained by "average other fighter compensation" or any other regressor in Dr.

Singer's model.<sup>39</sup> This is a substantial amount of unexplained variation that by definition represents deviation from a "structure" and would cause many pairs of individual fighters to have little correlation in their compensation.

### **VIII. DR. SINGER'S "COMMON FACTORS" REGRESSION MISTAKENLY ATTRIBUTES INDIVIDUALIZED SOURCES OF VARIATION TO COMMON FACTORS**

30. As further evidence of a compensation structure, Dr. Singer puts forward a regression analysis aimed at explaining "what proportion of the variation in Bout Class compensation (above and beyond the average) is attributable to common factors."<sup>40</sup> Dr. Singer regresses total fighter compensation (in levels) on the same explanatory variables in his impact regression, excluding certain "Fighter-specific performance variables."<sup>41</sup> He claims that the regression "therefore relies upon common and objectively measurable factors only—such as weight class, rank, gender, placement on the card, year, country, and venue—to explain variation in observed Fighter compensation."<sup>42</sup> He finds that his "common factors" explain "over three-quarters of the observed variation in Fighter Compensation—above and beyond what can be explained by the sample average," which he concludes is "evidence of a compensation structure determined by factors common to the Class as a whole."<sup>43</sup>

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<sup>39</sup> The "within R-squared" from Dr. Singer's compensation structure regression with the "average other fighter compensation" variable excluded is 0.294 (the within R-squared is the percentage of variation in compensation that is left after removing individual fighter fixed effects that is explained by the included regressors). The within R-squared from Dr. Singer's compensation structure regression with the "average other fighter compensation" variable included is 0.300. Thus, the increment to within R-squared due to the "average other fighter compensation" variable is very small at 0.006 and the remaining variation unexplained by the time trend and "average other fighter compensation" is  $1 - 0.30 = 0.70$ .

<sup>40</sup> Singer Report, ¶227.

<sup>41</sup> *Id.*

<sup>42</sup> *Id.*

<sup>43</sup> *Id.*

31. Dr. Singer’s arguments with respect to the common factors regression suffer from similar logical flaws as his compensation structure regressions. The only “common factor” that is relevant for the purposes of measuring individual injury due to the challenged conduct is the foreclosure share. Other “common factors” and their effects on fighter compensation are not relevant. Yet, incredibly Dr. Singer excludes the one relevant factor—the foreclosure share—from his common factor regression and focuses only on the irrelevant factors.<sup>44</sup> However, as discussed above in the context of the impact regression, the effect of foreclosure share in fact varies across individual fighters and thus is not “common.” Thus, even assuming that other supposedly common factors explain over three-quarters of variation in fighter compensation, this is irrelevant for the question of whether Dr. Singer’s impact regression can be used to determine individual injury resulting from the challenged conduct.

32. A further logical problem with Dr. Singer’s common factor regression is that some of the factors he claims are “common” are not in fact common. That is, these factors take on different values for different fighters and thus are individual factors. For example, Dr. Singer’s common factor regression includes the specific rank for each fighter at the time of the event (*Rank*), the number of prior fights and prior wins for the fighter (*Fights* and *Wins*), dummy variables for the number of prior bouts the fighter has fought for Zuffa (*Bout Number*), and other fighter-specific factors. Perhaps Dr. Singer will argue that the effects of these factors are common (the same) across individual fighters even if the values of the factors are individualized. Again, however, he

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<sup>44</sup> I note that if foreclosure share is added to Dr. Singer’s common factor regression, its estimated coefficient on Dr. Singer’s Tracked and Headliner definitions is statistically significant and positive (implying that a fighter’s compensation *increases* as Zuffa’s foreclosure share increases) and on Dr. Singer’s Ranked definition is statistically not significant and positive. These results contradict Dr. Singer’s conclusion that on aggregate fighters have been injured.

has not analyzed whether this is true, i.e., whether the effects of these factors are the same across individual fighters. I have demonstrated that the effect of foreclosure share varies across individual fighters, so there is no reason to believe that the effect of any other factor is the same for all fighters. To further analyze this question, I tested whether a supposedly common factor has the same effect across individual fighters. Using the same approach discussed above for the impact regression, I identified three explanatory variables in Dr. Singer's common factors regression that are among the most "statistically significant" as measured by their t-statistics (*WinID*, *LOA*, and *PPV*).<sup>45</sup> I allowed the effects of these variables to differ across all fighters, as well as across each fighter with at least 10 bouts and all other fighters combined.<sup>46</sup> I then statistically tested the hypothesis that the effect is the same across fighters. The test rejects the hypothesis.<sup>47</sup> It is therefore inappropriate for Dr. Singer to attribute to "common factors" variation that is in fact due to factors that are not common or that have non-common effects.

## **IX. DR. SINGER'S OTHER PURPORTED METHOD FOR DETERMINING INDIVIDUAL INJURY IS COMPLETELY FLAWED AND UNRELIABLE**

33. Dr. Singer claims that another method for determining individual injury, and establishing resulting class-wide impact, is to compare a fighter's actual Fighter Share to the prediction of the Fighter Share based on the impact regression with the foreclosure share variable set to its "but

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<sup>45</sup> For simplicity, I excluded from this test regressors with large sets of dummy variables.

<sup>46</sup> I run Dr. Singer's model, interacting fighters with the selected explanatory variable, separately for each selected explanatory variable.

<sup>47</sup> The *F*-statistics for the interactions for all fighters with *WinID*, *LOA*, and *PPV*, respectively, are 3.14, 4.04, and 4.15. The *p*-value is <0.00001% in all cases.

The *F*-statistics for the interactions for fighters with at least 10 bouts and all other fighters combined with *WinID*, *LOA*, and *PPV*, respectively, are 3.96, 7.79, and 7.45. The *p*-value is <0.00001% in all cases.

for,” rather than actual, value.<sup>48</sup> According to Dr. Singer, any fighter with predicted but-for revenue share greater than the actual revenue share for a given event was injured in that event. However—and this cannot be stated too strongly—Dr. Singer’s approach is completely flawed as a matter of economics and statistics. I note that he is unable to provide any support in the scholarly literature for his proposed approach and that is because there is none of which I am aware.

34. To understand why Dr. Singer’s method is flawed, it is informative to start by showing that Dr. Singer’s proposed method fails in the case of a “random controlled trial” (RCT). In an RCT, study subjects are randomly assigned to either the “treatment” group or the “control” group. The treatment group receives the “treatment” being studied (say, a drug that may be effective against high cholesterol) while the control receives no treatment (does not receive the drug). The “average treatment effect” is estimated by comparing the average outcome of interest (say, the change in cholesterol level) in the treatment group to the average outcome of interest in the control group. Note that, as with Dr. Singer’s impact regression, it is the average treatment effect across subjects that is being estimated by this procedure. The treatment effect may, in fact, be different across individual subjects, including being zero for some subjects.

35. Can the individual treatment effect for each subject in the treated group be determined reliably using Dr. Singer’s proposed method? Suppose that the average outcome in the control group is 0 (no change in cholesterol level). For some control group subjects, the outcome is positive and for other control group subjects it is negative, but the average across all control group subjects is zero. Suppose that the average outcome in the treated group is -10 (a 10 unit reduction

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<sup>48</sup> Singer Report, §IV.C. *See also*, Singer Rebuttal Report, §III.A. Dr. Singer also aggregates the resulting compensation levels (i.e., the product of Fighter Share and actual event revenues) across all events for each fighter to conclude whether an individual fighter was injured on net. *Id.*

in cholesterol level). Again, some treated group subjects have an outcome less than -10, while others have an outcome greater than -10, but the average outcome is -10.

36. The average treatment effect is then the difference between the average outcome in the treated and control groups, or  $-10 - 0 = -10$  (the drug reduces cholesterol by 10 units). Put another way, but-for the drug, the average outcome (change in cholesterol level) among the treated patients would be 10 units higher.

37. Suppose, however, that (unknown to the researcher) the treatment effect differs across patients (the drug works to reduce cholesterol in some patients, but not others). Although the drug's effect is -10 on average, the effect is zero for some patients and correspondingly greater in magnitude than -10 for others. Now consider a patient in the treatment group for whom the treatment effect is zero and suppose that this patient's outcome but-for the treatment, i.e., if the subject had (counterfactually) been in the control group, is -5 (a five unit reduction in cholesterol level for reasons having nothing to do with the drug). Note that, because the treatment has no effect for this subject by assumption, the subject's outcome with the treatment is also -5 (so that the difference in outcomes for this subject between the treated state and untreated (control) state is zero, consistent with the premise of this scenario that the drug has no effect on this patient).

38. We can examine what would happen if Dr. Singer's procedure were applied in this case. His procedure involves comparing the actual (treated) outcome of -5 for this subject to the average but-for (untreated) outcome of 0 (i.e., the average outcome of the control group). Thus, Dr. Singer's procedure would conclude that the effect of the treatment for this subject was  $-5 - 0 = -5$ , even though the individual's true treatment effect was zero. In other words, Dr. Singer's procedure gives the wrong answer. The procedure goes wrong because it fails to account for this subject having a better than average outcome due to factors having nothing to do with the drug. These

factors would carry over to the but-for (untreated) situation, so that in the but-for (untreated) world, the subject would also have an outcome better than the average outcome of the control group. However, Dr. Singer ignores this fact and instead assumes that the subject would be average (i.e., have the control average outcome of zero).<sup>49</sup>

39. This flaw in logic carries over to Dr. Singer’s impact regression as applied in this case. There, Dr. Singer claims to be able to determine individual injury by comparing the actual Fighter Share to the but-for prediction of the Fighter Share for each event from his impact regression.<sup>50</sup> This is a direct analogy to the RCT case described above because the actual Fighter Share is analogous to the outcome of a subject in the treatment group and the but-for prediction on the Fighter Share from Dr. Singer’s impact regression is analogous to the average outcome in the control group (i.e., removing the “treatment” of the challenged conduct).<sup>51</sup> I will now demonstrate directly that Dr. Singer’s proposed method suffers from the same flaw as in the RCT case. Under Dr. Singer’s impact regression formulation, the actual Fighter Share for a given fighter/event is:

$$\text{Actual Fighter Share} = \beta[\text{actual other variables}] + \delta[\text{actual foreclosure share}] + [\text{actual error}]$$

where “actual other variables” are the actual values of the other variables besides foreclosure share included in Dr. Singer’s impact regression, the actual error is the portion of the actual Fighter Share

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<sup>49</sup> The basic problem can be summarized as follows: for any one individual in the treatment group, it is not possible to determine whether that individual had a “good” outcome (relative to the control group) due to the treatment or due to some unobserved factor specific to that patient but unrelated to the treatment. It is only when we average over all the treatment group subjects that those unobserved individual-specific factors unrelated to the treatment average out to zero (approximately), leaving the average effect of the treatment across all treatment group subjects. But, again, even then we are only able to discern the average effect; we are not able to say anything about the individual effects, which may be zero for some individuals.

<sup>50</sup> Singer Report, §IV.C; Singer Rebuttal Report, §III.A.

<sup>51</sup> Recall that regression coefficients are just averages. So, the but-for prediction based on those regression coefficients is itself an average, just as the average outcome of the control group in an RCT is an average.

for the fighter/event that is not explained by the regression model, and  $\beta$  and  $\delta$  are the regression coefficients from the impact regression.

40. The but-for Fighter Share for the same fighter/event is:

$$\text{But-For Fighter Share} = \beta[\text{but-for other variables}] + \delta[\text{but-for foreclosure share}] + [\text{but-for error}]$$

41. The true level of injury for the fighter/event is the difference between the but-for and actual Fighter Shares for that fighter/event or:

$$\begin{aligned} \text{True Injury} = & \beta([\text{but-for other variables}] - [\text{actual other variables}]) + \\ & \delta([\text{but-for foreclosure share}] - [\text{actual foreclosure share}]) \\ & + ([\text{but-for error}] - [\text{actual error}]) \end{aligned}$$

42. This equation shows that the true level of injury for the fighter/event cannot be determined in general because the values of the inputs needed to calculate it are not all available to the researcher. Specifically, the but-for error is not observed, and indeed is unknowable, by the researcher because it corresponds to the counterfactual but-for world that did not actually occur. Because the but-for error cannot be determined, injury for the individual fighter/event cannot be calculated reliably.<sup>52</sup>

43. How does Dr. Singer get around the problem of not observing the but-for error and thereby claim to have determined injury for every individual fighter/event? *He assumes that the but-for error is zero for every fighter/event.* Making this assumption, plus the assumption that the “other

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<sup>52</sup> Even though injury for the individual fighter/event cannot be determined, average injury across fighter/events can still be determined, just like the average treatment effect in an RCT, because when the fighter/event injury equation is averaged over many fighter/events, the average of the but-for errors approaches zero and the average of the actual errors approaches zero.

variables” take on the same values in the but-for world as in the actual world,<sup>53</sup> the injury equation reduces to:

$$\text{Injury with Singer Assumptions} = \delta([\text{but-for foreclosure share}] - [\text{actual foreclosure share}]) - [\text{actual error}]$$

44. As can be readily seen, this equation corresponds to Dr. Singer’s proposed method of subtracting the actual Fighter Share for the fighter/event from the predicted but-for Fighter Share for that fighter/event obtained from the impact regression. The predicted but-for Fighter Share for the fighter/event from the impact regression is:

$$\text{Predicted But-For Fighter Share} = \beta[\text{actual other variables}] + \delta[\text{but-for foreclosure share}]$$

The actual Fighter Share for the fighter/event is:

$$\text{Actual Fighter Share} = \beta[\text{actual other variables}] + \delta[\text{actual foreclosure share}] + [\text{actual error}]$$

Therefore, Dr. Singer’s proposed method involves this calculation:

$$\begin{aligned} \text{Predicted But-For Fighter Share} - \text{Actual Fighter Share} &= \\ \delta([\text{but-for foreclosure share}] - [\text{actual foreclosure share}]) - [\text{actual error}] & \\ = \text{Injury with Singer Assumptions} & \end{aligned}$$

45. Dr. Singer’s proposed method is only as valid as the crucial assumption upon which it is based. However, this crucial assumption—that the but-for error is zero for every fighter/event—is entirely invalid and inappropriate.

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<sup>53</sup> This assumption may also be invalid. However, for the purposes of this declaration, I focus on the invalidity of Dr. Singer’s assumption that the but-for error is zero for all fighter/events.

46. First, it is unreasonable to assume that the but-for error is the same value of zero for every fighter/event. This would imply that the regression model perfectly fits the but-for Fighter Share for every fighter/event, an outcome that is essentially unheard of in empirical economics.

47. Second, as noted above, the but-for error corresponds to the but-for world, which never occurred. Dr. Singer has no basis—and can provide no basis—for claiming to know what value the but-for error would have taken on for any fighter/event, let alone that it would have been zero for every fighter/event, because he is not able to observe the but-for world.

48. Third, Dr. Singer makes inconsistent assumptions about the “other variables” in his impact regression and the error of the regression. While he assumes that the “other variables” take on the same values in the actual and but-for worlds, he assumes that the error takes on a different value in the but-for world (zero for all fighter/events) than in the actual world (where the error is generally non-zero). If he had instead assumed that the error, like the “other variables” in the regression, took on the same value in the but-for world as in the actual world, his injury equation would simplify further to:

*Injury with Consistent Singer Assumptions*

$$= \delta([but\text{-}for\text{ foreclosure share}] - [actual\text{ foreclosure share}])$$

However, in this case, Dr. Singer would be determining injury for individual fighter/events solely based on the average or aggregate foreclosure share coefficient from the impact regression. I have demonstrated above that individual fighters (and therefore fighter/events) have foreclosure share coefficients that vary from the average. Thus, an assessment of injury for an individual fighter (or a fighter/event) must be determined on the basis of that fighter’s coefficient, not the average. Put another way, if Dr. Singer were consistent in his assumptions, he would necessarily be assuming

that all fighter/events were affected in the same way, and then his method for determining individual injury would be an exercise in circularity.

49. Fourth, and relatedly, when the coefficient on foreclosure share varies across individual fighters, it can easily be seen that Dr. Singer's methodology leads to incorrect injury assessments. This is analogous to the example I gave in the case of an RCT. Consider a fighter for whom the foreclosure share had no effect, i.e., a fighter who has a foreclosure share coefficient of zero and, by definition, is uninjured. In Dr. Singer's impact regression, the foreclosure share has a negative coefficient based on the average across fighters. The actual error for the individual fighter in question would have to offset this negative average coefficient (given that foreclosure share has no effect on this individual fighter). This means that this individual fighter's actual error must be equal to:

$$\text{Actual Error} = -\delta[\text{actual foreclosure share}] + [\text{other actual error}]$$

Given this actual error, when Dr. Singer's methodology is applied to this individual fighter/event, the result is:

$$\begin{aligned} \text{Injury with Singer Assumptions} &= \delta([\text{but-for foreclosure share}] - [\text{actual foreclosure share}]) + \\ &\quad \delta[\text{actual foreclosure share}] - [\text{other actual error}] \\ &= \delta[\text{but-for foreclosure share}] - [\text{other actual error}] \end{aligned}$$

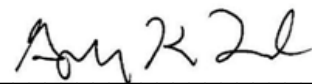
Because this fighter is uninjured, Dr. Singer's proposed method for determining injury for this individual fighter should produce zero injury if it is reliable. However, when the but-for foreclosure share is zero, Dr. Singer's approach would "find" (mistakenly) that this fighter was injured if the *other actual error* were negative. Given that the actual error would be expected to be negative about half the time (since regression error terms are distributed around a mean of zero),

Dr. Singer's approach would be wrong on uninjured fighters about half the time. Such an accuracy rate is poor.

## **X. CONCLUSION**

50. In conclusion, Dr. Singer has not provided a reliable method for determining individual injury. The economics literature recognizes that the effects of treatments can vary across individuals. Often economists are interested in the "average treatment effect" and the treatment effects literature has provided methods for estimating this average effect. However, I understand that an important question in this case is determining whether the challenged conduct had effects on each individual fighter, rather than just the average fighter. For that question, the literature has proposed a different set of methods. Dr. Singer's impact regression (at best) only addresses the question of average effect. It cannot answer the question of individual injury. Dr. Singer ignored the methods proposed in the literature for determining individual effects and instead attempts to bolster his argument with the compensation structure regression and common factors regressions. However, this attempt fails both as a matter of basic logic and empirically.

I declare under penalty of perjury under the laws of the United States that the foregoing is true and correct.



Executed on this 1<sup>st</sup> day of December in Hillsborough, California.

# Appendix A



**Gregory K. Leonard**  
Vice President

PhD, Economics  
Massachusetts Institute of Technology  
ScB, Applied Mathematics-Economics  
Brown University

Dr. Gregory K. Leonard is a vice president in the Antitrust & Competition Economics Practice of CRA. He specializes in applied microeconomics and econometrics. He has provided testimony before US federal and state courts, government agencies, and arbitration panels on issues involving antitrust, damages estimation, statistics and econometrics, surveys, valuation, and labor market discrimination.

Dr. Leonard has written extensively in the areas of antitrust, industrial organization, econometrics, intellectual property, class certification, and labor economics. His publications have appeared in journals such as the *RAND Journal of Economics*, the *Journal of Industrial Economics*, the *Journal of Econometrics*, the *International Journal of Industrial Organization*, and the *Antitrust Law Journal*, among others. Dr. Leonard's writings were cited by the Court of Appeals for the Federal Circuit in its *Uniloc* decision and his trial testimony was cited by the Supreme Court in its *Oracle v. Google* decision. He is the Editorial Board Vice Chair for Economics of the *Antitrust Law Journal* and has served as a referee for numerous economic journals.

Dr. Leonard has given invited presentations on antitrust and intellectual property issues at the (US) Federal Trade Commission, the US Department of Justice, the former Anti-Monopoly Bureau of China's Ministry of Commerce, the Supreme People's Court of China, and Japan's Fair Trade Commission. He served as a consultant on the issue of immunities and exemptions to the (US) Antitrust Modernization Commission.

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Discussant, “New Developments in Antitrust” session, AEA meetings, January 7, 2000.

“In Defense of Merger Simulation,” Department of Justice and Federal Trade Commission Merger Workshop, Unilateral Effects Session, February 18, 2004.

Discussant, “Proving Damages in Difficult Cases: Mock Trial & Discussion,” NERA Antitrust & Trade Regulation Seminar, July 10, 2004.

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Discussant, “Dominance and Abuse of Monopoly Power” Session, China’s Competition Policy and Anti-Monopoly Law, J. Mirrlees Institute of Economic Policy Research, Beijing University, and the Research Center for Regulation and Competition, Chinese Academy of Social Sciences, Beijing, People’s Republic of China, October 14, 2007.

“Opening Remarks,” Seminar on China’s Anti-monopoly Law and Regulation on Abuse of Intellectual Property Rights, Beijing, People’s Republic of China, April 26, 2008.

“Issues to Consider in a Reasonable Royalty Damages Analysis,” Patent Litigation 2008, Practising Law Institute, October 7, 2008.

“Econometric Evaluation of Competition in Local Retail Markets,” Federal Trade Commission and National Association of Attorneys General Retail Mergers Workshop, December 2, 2008,

“Merger Review Best Practices: Competitive Effects Analysis,” International Seminar on Anti-Monopoly Law: Procedure and Substantive Assessment in Merger Control, Beijing, People’s Republic of China, December 15-17, 2008.

“The Use of Natural Experiments in Antitrust,” Renmin University, Beijing, People’s Republic of China, December 18, 2008.

“China’s Antimonopoly Law: An Economist’s Perspective,” Bloomberg Anti-Monopoly Law of China Seminar, January 29, 2009.

Panelist, “Standards for Assessing Patent Damages and Their Implementation by Courts,” FTC Hearings on the Evolving IP Marketplace, February 11, 2009.

“Economic Analysis of Agreements Between Competitors” and “Case Study: FTC Investigates Staples’ Proposed Acquisition of Office Depot,” Presentation to Delegation of Antitrust Officials from the People’s Republic of China, Washington, DC, March 23, 2009.

“Reasonable Royalties in the Presence of Standards and Patent Pools,” LSI Workshop, April 20, 2009.

Presentations on Unilateral Effects, Buyer Power, and the Intellectual Property-Antitrust Interface to Delegation from the Anti-Monopoly Bureau of MOFCOM of the People’s Republic of China, Washington, DC, May 10-11, 2009.

Panelist, “The Use of Economic and Statistical Models in Civil and Criminal Litigation,” Federal Bar Association, San Francisco, May 13, 2009.

“Trends in IP Rights Litigation and Economic Damages in China,” Pursuing IP in the Pacific Rim, May 14, 2009.

Presentation on the Economics of Antitrust, National Judicial College of the People’s Republic of China, Xi’an, People’s Republic of China, May 25-26, 2009.

“Case Study: The Use of Economic Analysis in Merger Review,” Presentation to the Anti-Monopoly Bureau of MOFCOM, Beijing, People’s Republic of China, May 27, 2009.

“Economics and Antitrust Law,” China University of Political Science and Law, Beijing, People’s Republic of China, September 21, 2009.

“Case Study: Economic Analysis of Coordinated Interaction,” Presentation to the Anti-Monopoly Bureau of MOFCOM, Beijing, People’s Republic of China, September 22, 2009.

“Relevant Market Definition,” 4<sup>th</sup> Duxes Antitrust Law Seminar, Beijing, People’s Republic of China, September 26, 2009.

“Expert Economic Testimony in Antitrust Litigation,” Supreme People’s Court, Beijing, People’s Republic of China, February 2, 2010.

“New Case Law for Patent Damages,” Law Seminars International Telebriefing, April 28, 2010.

“China/India: Sailing in Uncharted Waters: Regulating Competition in the Emerging Economies – New Laws, New Enforcement Regimes and No Precedents,” The Chicago Forum on International Antitrust Issues, Northwestern University School of Law Searle Center, May 20, 2010.

“Antitrust and Intellectual Property,” Supreme People’s Court, Beijing, People’s Republic of China, May 26, 2010.

“Cartel Enforcement Trends in the United States,” 2<sup>nd</sup> Ethical Beacon Anti-Monopoly Summit, Beijing, People’s Republic of China, May 27, 2010.

Panelist, “The Future of Books and Digital Publishing: the Google Book Settlement and Beyond,” 2010 American Bar Association Annual Meeting, August 7, 2010.

“Coordinated Effects” and “Non-Horizontal Mergers,” Presentations to Delegation from India Competition Commission, US Chamber of Commerce, Washington, DC, October 26, 2010.

“UPP and Merger Simulation,” Annual Conference of the Association of Competition Economics, Norwich, UK, November 11, 2010.

“Uniloc v. Microsoft: A Key Ruling For Patent Damages,” Law Seminars International Telebriefing, January 21, 2011.

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“Private Litigation Under China’s New Antimonopoly Law,” Bar Association of San Francisco, February 17, 2011.

“Competition Law and State Regulation: Setting the Stage and Focus on State-Owned Enterprises,” Competition Law and the State: International and Comparative Perspectives, Hong Kong, People’s Republic of China, March 18, 2011.

Panelist, “Booking it in Cyberspace: The Google Book Settlement and the Aftermath,” American Intellectual Property Law Association, San Francisco, May 13, 2011.

“Econometric Estimation of Cartel Overcharges,” ZEW Conference on Economic Methods and Tools in Competition Law Enforcement, Mannheim, Germany, June 25, 2011.

Panelist, "Antitrust and IP in China," Antitrust and IP in Silicon Valley and Beyond, American Bar Association and Stanford University, Palo Alto, October 6, 2011.

Panelist, University of San Diego School of Law Patent Law Conference: The Future of Patent Law Remedies, January 18, 2013.

"Economics Framework," US-China Workshop on Competition Law and Policy for Internet Activities, China's State Administration for Industry and Commerce (SAIC) and the U.S. Trade and Development Agency (USTDA), Shenzhen, People's Republic of China, June 4-5, 2013.

Panelist, "China Inside and Out," American Bar Association, Beijing, People's Republic of China, September 16-17, 2013.

Panelist, "Remedies in Patent Cases," Fifth Annual Conference on The Role of the Courts in Patent Law & Policy, Berkeley and Georgetown Law Schools, November 1, 2013.

"Royalty Base," Leadership Conference, Qualcomm Incorporated, March 21, 2014.

"Reflections on Natural Experiments," DG Comp, April 8, 2014.

Panelist, "Antitrust in Asia: China," American Bar Association Section of Antitrust Law, Beijing, People's Republic of China, May 21-23, 2014.

Panelist, "Patent Damages Roundtable," 2015 Intellectual Property Institute, University of Southern California Gould School of Law, Los Angeles, March 23, 2015.

Panelist, "IP and Antitrust – The Current State of Economic Analysis," Global Competition Review Live 2nd Annual IP & Antitrust USA, Washington, DC, April 14, 2015.

Panelist, "FRAND Royalty Rates After Ericsson v. D-Link," American Bar Association, May 15, 2015.

Participant, Patent Damages Workshop, University of California-Berkeley, March 3, 2016.

Panelist, "FRANDtopia – In a Perfect World," LAIPLA Spring Conference, May 5, 2018.

Panelist, "Chicago Forum on International Antitrust Issues," Northwestern Pritzker School of Law, June 15, 2018.

Panelist, "Competition in Digital Advertising: Is There Online and Offline Convergence?," Challenges to Antitrust in a Changing Economy, Harvard Law School, November 8, 2019.

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*Press Ganey Associates, Inc. v. Qualtrics, LLC*, American Arbitration Association, Case No. 01-18-0004-4674, 2019 (Deposition).

*In the Matter of: Determination of Rates and Terms for Digital Performance of Sound Recordings and Making of Ephemeral Copies to Facilitate those Performances (Web V)*, before the United States Copyright Royalty Board Library of Congress, Docket No. 19-CRB-0005-WR (2021-2025), 2020 (Deposition, Trial Testimony).

*Abiomed Inc. v. Maquet Cardiovascular LLC*, United States District Court for the District of Massachusetts, Case No. 1:16-cv-10914-FDS, 2020 (Deposition).

*Network-1 Technologies, Inc. v. Google LLC*, United States District Court for the Southern District of New York, Case No. 1:14-cv-09558, 2020 (Deposition).

*3Shape A/S v. Align Technology, Inc.*, United States District Court for the District of Delaware, Civil Action No. 18-886-LPS-CJB, 2020 (Deposition).

*District Council #16 Northern California Health & Welfare Trust Fund v. Sutter Health, et al*, No. RG15753647, 2021 (Deposition).

*In the Matter of Certain Digital Video-Capable Devices and Components Thereof*, Investigation No. 337-TA-1224, United States International Trade Commission, 2021 (Deposition).

*Teradata US, Inc., Teradata Corporation and Teradata Operations, Inc. v. SAP SE, SAP America, Inc. and SAP Labs LLC*, United States District Court for the Northern District of California, Case No. 3:18-cv-03670-WHO, 2021 (Depositions).

*American Society of Composers, Authors and Publishers v. Radio Music License Committee*, Arbitration, 2021 (Hearing Testimony).

*PureWick Corporation v. Sage Products, LLC*, United States District Court for the District of Delaware, Case No. 1:19-cv-01508-MN, 2021 (Deposition), 2022 (Trial Testimony).

*In Re Caustic Soda Antitrust Litigation*, United States District Court for the Western District of New York, Case No. 1:19-cv-003895-EAW-MJR, 2022 (Deposition).

*Bedford, Freeman & Worth Publishing Group, LLC d/b/a Macmillan Learning, Macmillan Holdings, LLC, Cengage Learning, LLC, Elsevier Inc., Elsevier B.V., McGraw Hill LLC, and Pearson Education Inc. v. Shopify, Inc.*, United States District Court for the Eastern District of Virginia, Case 1:21-cv-01340, 2022 (Deposition).

*Athos Overseas, Ltd. v. YouTube, LLC and Google LLC*, United States District Court for the Southern District of Florida, Miami Division, Case No. 1:21-cv-21698-DPG, 2022 (Deposition).

*In Re Google Play Store Antitrust Litigation*, United States District Court for the Northern District of California, San Francisco Division, Case No. 3:21-md-02981-JD, Case No. 3:21-cv-05227, 2023 (Deposition).

*Broadcom Corporation, et al. v. Netflix, Inc.*, United States District Court for the Northern District of California, Case No. 3:20-cv-04677-JD (N.D. Cal.), 2023 (Deposition).

## Professional activities

Member, American Economic Association

Member, Econometric Society

Member, American Bar Association

Contributor, [www.antitrust.org](http://www.antitrust.org)

Contributor, ABA Section of Antitrust Law, *Econometrics*, 2005

Associate Editor, *Antitrust*, 2007-2010

Senior Editor, *Antitrust Law Journal*, 2012-; Associate Editor, 2010-2012

Co-Editor, ABA Section of Antitrust Law Economics Committee Newsletter, 2009-2012

Member, Economics Task Force, ABA Section of Antitrust Law, 2011-2012

Member, ABA Delegation to International Seminar on Anti-Monopoly Law: Procedure and Substantive Assessment in Merger Control, Beijing, People's Republic of China, December 15-17, 2008.

Member, Working Group for drafting the "Joint Comments of the American Bar Association Section of Antitrust Law and Section of International Law on the MOFCOM Draft Guidelines for Definition of Relevant Markets," 2009.

Member, Working Group for drafting the "Joint Comments of the American Bar Association Section of Antitrust Law and Section of International Law on the SAIC Draft Regulations on the Prohibition of Acts of Monopoly Agreements and of Abuse of Dominant Market Position," 2009.

Member, Working Group for drafting the "Joint Comments of the American Bar Association Section of Antitrust Law and Section of International Law on the SAIC Draft Regulations on the Prohibition of Acts of Monopoly Agreements and of Abuse of Dominant Market Position," 2010.

Referee: *Econometrica*, *Review of Economics and Statistics*, *International Journal of Industrial Organization*, *Review of Industrial Organization*, *Journal of Sports Economics*, *Journal of Environmental Economics and Management*, *Research in Law and Economics*, *Labour Economics*, *Eastern Economic Journal*, *Journal of Forensic Economics*, *Antitrust*, *Antitrust Law Journal*, *Journal of Competition Law and Economics*, *Advances in Econometrics*.

## Professional history

12/2019–Present	<i>Vice President</i> , Charles River Associates
2012–2019	<i>Partner</i> , Edgeworth Economics
2008–2012	<i>Senior Vice President</i> , NERA Economic Consulting
2004–2008	<i>Vice President</i> , NERA Economic Consulting
2000–2004	<i>Senior Vice President</i> , Lexecon, Inc.
1991–2000	<i>Director</i> , Cambridge Economics, Inc.
1990–1991	<i>Senior Analyst</i> , NERA Economic Consulting

1989–1990

*Assistant Professor*, Columbia University

- Econometrics
- Statistics
- Labor Economics

# Appendix B

## **Appendix B**

### **Documents Relied Upon**

#### **Academic Articles and Books**

David Spector, "Exclusive Contracts and Demand Foreclosure," RAND Journal of Economics (2011), pp. 619-638.

Guido W. Imbens and Jeffrey M. Wooldridge, "Recent Developments in the Econometrics of Program Evaluation," Journal of Economics Literature, 2009, pp. 5-86.

Jeffrey M. Wooldridge, *Introductory Econometrics: A Modern Approach*, 7th edition, Cengage Learning.

John H. Johnson and Gregory K. Leonard, "Rigorous Analysis of Class Certification Comes of Age," Antitrust Law Journal, 2011, pp. 569-586.

Murillo Campello, et al., "Testing for Slope Heterogeneity Bias in Panel Data Models," Journal of Business and Economic Statistics, 2019, pp. 749-760.

#### **Expert Reports**

Expert Rebuttal Report of Andrew Zimbalist (December 26, 2017).

Expert Rebuttal Report of Professor Alan Manning (January 12, 2018).

Expert Report of Andrew Zimbalist (August 30, 2017).

Expert Report of Elizabeth Kroger Davis (October 27, 2017).

Expert Report of Guy A. Davis, CPA, CIRA, CDBV, CFE (August 31, 2017).

Expert Report of Hal J. Singer, Ph.D. (August 31, 2017).

Expert Report of Paul Oyer (October 27, 2017).

Expert Report of Professor Robert H. Topel (October 27, 2017).

Expert Report of Richard Marks (October 27, 2017).

Expert Report of Roger D. Blair (November 15, 2017).

Professor Robert H. Topel's Reply to the Supplemental Expert Report of Hal J. Singer, Ph.D. (May 7, 2018).

Rebuttal Expert Report of Guy A. Davis, CPA, CIRA, CDBV, CFE (January 12, 2018).

Rebuttal Expert Report of Hal J. Singer, Ph.D. (January 12, 2018).

Second Supplemental Expert Report of Hal J. Singer, Ph.D. (May 28, 2018).

Supplemental Expert Report of Hal J. Singer, Ph.D. (April 3, 2018).

Sur-Rebuttal Expert Report of Professor Robert H. Topel (February 12, 2018).

Errata from all experts and materials in the turnover production from Dr. Singer and Dr. Topel are incorporated by reference.